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# **Do negative random shocks affect trust and trustworthiness?**

**Hernán Bejarano • Joris Gillet • Ismael Rodriguez-Lara**

## **Abstract**

We report data from a variation of the trust game aimed at determining whether (and how) inequality and random shocks that affect wealth influence the levels of trust and trustworthiness. To tease apart the effect of the shock and the inequality, we compare behavior in a trust game where the inequality is initially given and one where it is the result of a random shock that reduces the second mover's endowment. We find that first-movers send less to second-movers but only when the inequality results from a random shock. As for the amount returned, second-movers return less when they are endowed less than first-movers, regardless of whether the difference in endowments was initially given or occurred after a random shock.

**Keywords:** Trust game, endowment heterogeneity, random shocks, inequality aversion, experimental economics.

**JEL Codes:** C91, D02, D03, D69.

## 1. Introduction

Trust and trustworthiness are key factors in promoting cooperation and exchange (e.g., Smith 1776, Arrow 1974, Guiso et al. 2004). Also, it is well documented that they have a positive impact on government functioning and economic growth rates (Arrow 1974, Knack & Keefer 1997, Gambetta 1988, Zak & Knack 2001; Bjørnskov 2012). While the determinants of trust and trustworthiness have been extensively studied (see Johnson & Mislin 2011, Eckel & Wilson 2011, Cooper & Kagel 2013 for recent surveys), little is known on the effects of negative random shocks on the levels of trust and trustworthiness.<sup>1</sup> Negative external chance events might occur because of man made calamities (e.g., war, cycles of boom and bust), natural disasters (e.g., earthquakes) or personal shocks (e.g., health problems). These events do not only affect wealth directly but are also likely to alter the levels of trust and trustworthiness among the members of a community. Because identifying these effects is difficult or unlikely in the field, we investigate this question in a laboratory setting.

In our innovative design, we consider a variation of the trust game (Berg et al. 1995) in which first- and second-movers are initially endowed with the same amount but the outcome of a die roll can result in decreasing the second-mover's endowment before the decision of the first-mover.<sup>2</sup> As a result, first-movers can face either a second-mover that has the same endowment or a second-mover who started with the same endowment but has lost part of it as the result of a shock. We keep the endowment of the first-mover constant, so our experiment is designed to investigate if first-movers' transfers (that generate efficiency gains and measure the level of trust) are affected when second-movers suffer a negative shock that affects their wealth. We also aim to study whether shocks affect the level of trustworthiness; i.e., the willingness to reciprocate by second-movers. In addressing these questions, we isolate the effects of the negative random shock from the inequality it generates by considering two additional treatments without shocks: one in which first- and second-movers are initially given the same endowment, another in which second-movers are initially endowed with less than the first-movers. This way, we can determine what harms trust and trustworthiness more: the mere presence of inequalities or experiencing the act of this inequality coming into existence.

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<sup>1</sup> The literature studies different motivations behind trust and trustworthiness. These include, among others, the role of other-regarding preferences (Cox 2004, Ashraf et al. 2006, Kanagaretnam et al. 2009, Cox et al. 2016), risk aversion (Eckel & Wilson 2004, Ashraf et al. 2006, Houser et al. 2010, Kanagaretnam et al., 2009) and cognitive abilities (Corgnet et al. 2015).

<sup>2</sup> In the trust game, the first-mover has to decide what part of her endowment (if any) she wants to send to the second-mover. The experimenter multiplies the amount sent by three before the second-mover can decide how much to send back. Because the sub-game perfect equilibrium under the assumption of purely self-interested subjects is that second-movers will return nothing to the first-movers, the first-mover's decision has been usually identified in the literature as the level of trust, whereas the level of trustworthiness has been frequently measured by how much the second-mover returns.

We focus on outcome-based models to posit some behavioral predictions on the behavior of the first- and the second-mover. For example, models of inequality-aversion (Fehr & Schmidt 1999, Bolton & Ockenfels 2000) predict that second-movers return less when their endowment is lower, but do not yield different predictions depending on whether the inequality is initially given or the result of the random negative shock. We build on the idea of reference-dependent utility (Kőszegi & Rabin, 2006, 2007, 2009; Abeler et al., 2011) and assume that second-movers might use their initial endowment as a reference point. In this setting, the occurrence of the random shock will be perceived as a loss in utility for second-movers, compared with the case in which the inequality is initially given. Incorporating the idea of reference-dependent utility leads to the prediction that second-movers will return less in an unequal situation where the inequality is the consequence of a shock than in a situation where the inequality was initially given.

The existence of endowment heterogeneity in the trust game relates our work to a strand of the experimental literature that investigates the effects of inequality on trust and trustworthiness. Anderson et al. (2006) study the trust game introduced in Berg et al. (1995) and give subjects different show-up fees. They find no effect of such induced inequality on the amount sent by first-movers. Two additional articles support the result that induced inequalities do not affect trust. Hargreaves-Heap et al. (2013) use the design in Anderson et al. (2006) but manipulate the extent to which inequality is known to subjects. Brülhart & Usunier (2012) give subjects the same show-up fee but vary the amount that first-movers receive to play the trust game. There is also experimental evidence that endowment heterogeneity affects behavior in the trust game. For example, Lei & Vesely (2010), Smith (2011) and Rodriguez-Lara (2015) find (weak) evidence that first-movers send more to second-movers when the latter have a larger endowment. As for the behavior of second-movers, Xiao & Bicchieri (2010) find that they return less when they start out poorer than the first-mover compared to an equal distribution (see also Smith 2011). This is in line with Ermisch & Gambetta (2016), who run an experiment using subjects from the British Household Panel Survey and find that a decrease in the respondent's wealth (when comparing current and past income) reduces trustworthiness.

The current paper contributes to the experimental literature by looking at the effects of random shocks on trust and trustworthiness, an issue that has so far not been given any significant attention in laboratory settings. In this respect, our experimental design allows us to study whether or not subjects care about the *source* of the inequality; i.e., we can tease apart the effect of inequality that occurred *ex-post* (i.e., after the random shock) from *ex-ante* inequality (i.e., an inequality that was initially given). Almas et al. (2010, 2016) and Cappelen et al. (2013) investigate of how subjects treat inequalities that are the result of a random process in a

series of dictator game experiments.<sup>3</sup> The authors find that way in which the inequality is generated has an effect on redistribution, as it is also shown in Cherry et al. (2002) or Durante et al. (2014). In these papers, the inequality is not initially given or the result of a random process, but instead depends on the subjects' performance in a real-effort task; i.e., the inequality is *earned* by subjects. One aspect that makes our paper divert from this literature is that the decision to transfer to others in these games does not entail any efficiency gains, as it occurs in our setting: Along these lines, we consider a setting where transfers represent "altruism plus", as suggested by Cox (2004) or Cox et al. (2016).

Our experimental data suggest that it is the occurrence of the random shock (and not the existence of inequalities) what may hamper trust behavior and result in efficiency losses; i.e., first-movers send less when second-movers suffer a shock. Our findings reject the hypothesis that first-movers have utilitarian preferences or are purely altruistic. As for second-movers, we find that they return less to first-movers in the presence of the inequality, but they do not behave differently with and without the shock; i.e., our data for second-movers seems to be consistent with models of inequality aversion.

We speculate that first-mover's behavior might take place under the belief that second-movers are inequality-averse and have reference-dependent utility. This idea relates our paper to Bohnet et al. (2010), who suggest that differences in trust between Gulf and Western countries may be explained by different expectations of trustworthiness levels in these countries. In fact, there is evidence that expectations of trustworthiness are key to explain trust behavior (Ashraf et al. 2006, Bohnet 2007). We argue that first-movers might trust less when the inequality occurs after a random shock, because they may anticipate that second-movers will be less trustworthy when negative chance events occur.

Our results complement the survey data in Alesina & La Ferrara (2002), where it is shown that traumatic personal experiences or "shocks" (e.g., diseases, divorce or financial problems) have a negative effect on trust, as measured in the General Social Survey (GSS).<sup>4</sup> Our experimental work dovetails also with field studies that investigate trust and trustworthiness after natural disasters (Castillo & Carter 2011, Fleming et al. 2014, Andrabi & Das 2016, Cassar et al. 2017). Overall, field studies that investigate trust and trustworthiness in regions affected by a natural disaster mixed evidence. For example, Cassar et al. (2017) find that villages affected

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<sup>3</sup> Somewhat related, Selten & Ockenfels (1998) find that winners of a lottery are willing to transfer some money to losers in a three-player solidarity game.

<sup>4</sup> Using the GSS, Alesina & La Ferrara (2002) also show that trust is lower in more heterogeneous societies. Glaeser et al. (2000) is the seminal paper investigating the relationship between the GSS and the behavior in the trust game. See Aksoy et al. (2018) for a recent discussion.

by a tsunami in Thailand exhibit higher levels of trust and trustworthiness than non-affected villages. On the other hand, Fleming et al. (2014) show that trust does not differ between areas that were and were not affected by an earthquake in Chile; but that trustworthiness was indeed lower in the affected regions.<sup>5</sup> By using a laboratory setting, we can eliminate the usual problems of information and beliefs about the wealth of other subjects, which might be confounding factors in the field. Fleming et al. (2014) or Kanagaretman et al. (2009) argue that aftermath moral hazard (or asymmetric information about the damages suffered after a natural disaster) can affect the levels of trust and trustworthiness. While our experiment controls for this feature, we are not interested in looking at the effects on trust and trustworthiness when *both* players might be affected by the random shock. Instead, we investigate whether first-movers trust more to those who were affected by a random shock that affects their wealth, compared with a situation in which inequalities were initially given. We also investigate how second-movers respond to inequalities that are initially given and those that occur by negative chance event.

The remainder of the paper is arranged as follows. In Section 2, we present our experimental design and the hypotheses. We report our results in Section 3. Section 4 concludes.

## **2. Experimental design and hypotheses**

### **2.1. Experimental Design**

The experiment was conducted at ESI Chapman University. A total of 346 students (with no previous experience in similar experiments) were recruited to participate in 15 sessions from April 2014 to December 2017.

We place first- and second-movers in different rooms and use the standard procedures for hand-run trust experiments. All subjects received their initial endowment in a sealed envelope. First-movers were asked to decide the amount of money they wanted to send (if anything) to their matched second-mover. The amount sent was tripled by the instructor and then given to second-movers, who were asked to decide the amount to send back (if anything) to their matched first-mover.

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<sup>5</sup> The natural experiment in Veszteg et al. (2015) is an exception in this literature. The authors elicited behavior in the trust game using a survey that was carried out before and after an earthquake and tsunami hit a region in Japan. While the authors do not find a significant difference in trust behavior, they find that responders who completed the survey after the disaster were more trustworthy. See Toya and Skidmore (2014) for empirical evidence on the importance of natural disasters in trust and Tol and Leek (1999) for the negative consequences of natural disasters to economic growth. For the effects of natural disasters on other factors such as risk preferences, the interested reader can consult Eckel et al. (2009), Cameron & Shah (2015), Said et al. (2015) or Cassar et al. (2017).

**Table 1.** Summary of treatment conditions

Treatment	N	Initial endowments		
		First-mover	Second-mover	
Equal	47	21 E\$	21 E\$	
Unequal	47	21 E\$	7 E\$	
Shock Equal	39	21 E\$	21 E\$ → 21 E\$	The outcome of the die was odd and the second-mover kept her initial endowment.
Shock Unequal	40	21 E\$	21 E\$ → 7 E\$	The outcome of the die was even and 14 E\$ were reduced from the initial endowment of the second-mover.

*Note.* N refers to the number of pairs. Experimental Dollars (E\$) were converted to actual dollars at the end of each session (1 E\$ = \$0.50)

We summarize our treatment conditions in Table 1, which includes information about the number of pairs in each treatment. We had two control treatments without shocks (Equal & Unequal) and two other treatments depending on how chance events affected the endowment of the second-movers (Shock-Equal & Shock-Unequal). In all treatments, first-movers received 21 E\$.<sup>6</sup> In the Equal (Unequal) treatment, second movers received 21 E\$ (7E\$), respectively. In the treatments where the shock was possible second-movers were initially endowed with 21E\$. Before first-movers made their choices, the instructor rolled a die in front of each of them individually. If the number was odd, the second-movers kept their initial endowment (Shock-Equal). Otherwise, 14 E\$ were deducted from their initial endowment (Shock-Unequal). We wanted to make the shock salient to both players, so we asked first-movers to record the outcome of the die roll in a result card. The experimental coordinator collected these cards and the envelopes and proceeded to the other room. In that room, Players B realized the outcome of the die and were reduced their endowments before making their decision about the amount to return. Our game was played once; i.e., we do not allow for repetition. This was to mimic the occurrence of random events (e.g., natural disasters), which usually occur once.<sup>7</sup>

## 2.2. Hypotheses

Let  $e_i$  denote the level of endowment of player  $i \in \{1, 2\}$ , where  $i = 1$  ( $i = 2$ ) stands for the first-mover (second-mover) and  $e_1 \geq e_2 > 0$ . The first-mover decides the amount  $X \in [0, e_1]$  to send to the second mover, who has to choose the percentage  $y \in [0, 1]$  of the available

<sup>6</sup> We use Experimental Dollars (E\$) in our experiment. These were converted to actual dollars at the end of each session (1 E\$ = \$0.50). See Appendix A for the experimental instructions.

<sup>7</sup> Studying the effects of frequent shocks (i.e., in repeated trust games) seems a very interesting research line but it is outside of the scope of our current paper, as the repetition could (endogenously) affect the reference point of subjects in our experiment.

funds to return. We denote  $\pi_i$  the final payoffs of each of player  $i \in \{1, 2\}$ , which is determined as follows:

$$(1) \pi_1 = e_1 + X(3y - 1)$$

$$(2) \pi_2 = e_2 + 3X(1 - y)$$

In what follows, we derive our predictions regarding the behavior of the first and the second-movers.<sup>8</sup> If participants only cared about their own payoffs second-movers would never choose a positive return which, if first-movers used backward induction, would mean that they would never send anything. The fact that we do observe trust and trustworthiness in trust game experiments (Chaudhuri & Gangadharan 2007, Eckel & Wilson 2011, Johnson & Mislin 2011, Cooper & Kagel 2013) highlights the role of other-regarding preferences; i.e., players seem to care about the payoff of the other player.

Consider first the possibility of altruistic preferences. We say that player  $i$  has altruistic preferences if the player's utility function can be written as follows:

$$(3) u_i = \pi_i + b_i \pi_j$$

where  $\pi_i$  and  $\pi_j$  are given by equations (1) and (2) and  $b_i > 0$  measures the extent to which player  $i$  cares about the payoff received by the other player  $j$  for  $i, j \in \{1, 2\}, i \neq j$ . We assume that player  $i$  cares about his or her own payoff more than he or she cares about the payoff of the other player ( $b_i < 1$ ). We also consider that utilitarian or efficiency preferences can be viewed as a particular case of equation (3) for the case in which players want to maximize the total payoffs; i.e.,  $b_i = 1$ . In this setting, the first-mover should send nothing to the second-mover if  $b_1 \in (0, 1/3)$ . Otherwise, it is optimal for the first-mover to send everything to the second-mover when  $b_1 \geq 1/3$ . The logic of this argument holds because any amount sent by first-movers is multiplied by 3 and results in efficiency gains, thus a first-mover that cares sufficiently about the payoff of the second-mover (or has utilitarian preferences) should send everything to the second-mover. When we look at the second-mover, we should notice that any amount returned to the first mover does not entail any efficiency gains, therefore the second-mover should return nothing to the first mover unless  $b_2 = 1$ . Importantly, the utility

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<sup>8</sup> We consider outcome-based models but there are also intention-based models that look at the role of intentions and beliefs on the kindness of other players. See Rabin (1993), Dufwenberg & Kirchsteiger (2004) and Cox et al. (2007) for theoretical models of this kind and McCabe et al. (2003) or Cox et al. (2016) for experimental evidence.



function of altruistic players in (3) is linear in the level of endowments, thus changes in the endowments should not affect the behavior of the first or the second-mover.<sup>9</sup>

**Prediction 1.** *Altruistic preferences of the first-movers predict that they will send nothing or everything to the second-mover, depending on the value of  $b_1 \in (0, 1)$ ; in fact, first-movers should send everything if they have utilitarian preferences and  $b_1 = 1$ . If second-movers have altruistic preferences, they will return nothing to the first-mover, unless  $b_2 = 1$ . The behavior of the first- and second-mover will be unaffected by the level of endowments.*

One prominent idea in the literature on other-regarding preferences is that subjects dislike payoff differences (Fehr & Schmidt 1999, Bolton & Ockenfels 2000). Next, we move to the possibility of inequality-averse players, who are assumed to have the utility function:

$$(4) u_i = \pi_i - \alpha_i (\pi_i - \pi_j)^2$$

where  $\pi_i$  and  $\pi_j$  are given by equations (1) and (2) and  $\alpha_i \geq 0$  measures the extent to which player  $i$  is concerned about the inequality. Using our functional form in (4), it can be shown that the optimal amount send by an inequality-averse first-mover depends on the return they expect to receive from the second-movers and the difference between the levels of endowments. We follow Brülhart & Usunier (2011) and assume that first-movers who care about the inequality do not expect to receive anything back from second-movers.<sup>10</sup>

$$(5) X^* = \frac{e_1 - e_2}{4} - \frac{1}{32 \alpha_1}$$

The optimal behavior of inequality-averse first-movers who do not care about the level of trustworthiness is given by equation (5). This, in turn, implies that first-movers will send more in the Unequal and the Shock-Unequal treatment, compared with the Equal and Shock-Equal treatment. As for the behavior of inequality-averse second-movers, we can derive their optimal behavior from equation (4) as well. In their case, they should return a proportion of the available funds that takes into account both the amount that they receive from the first-mover ( $X$ ) and the difference in endowments. In particular, any difference in the initial endowments in favor of the first-mover will decrease the amount that the second-movers return.

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<sup>9</sup> Note that the value of  $b_i > 0$  is assumed to be independent on the levels of endowment.

<sup>10</sup> We acknowledge that this is a simplifying assumption but it avoids that the behavior of the first-mover depends the extent to which the first-mover believes that the second-mover cares about the inequality. The prediction also holds if the first-mover believes that the second-mover is altruistic.

$$(6) y^* = \frac{2}{3} - \frac{1}{12 X \alpha} - \frac{e_1 - e_2}{6X}$$

If first-movers are self-interested and anticipate the behavior of inequality-averse second-movers, they will send less in treatments with inequalities. We summarize these predictions as follows:

**Prediction 2.** *If first-movers are inequality-averse, they will send more when the endowment of the second-mover is lower (Unequal and Shock-Unequal treatments).*

**Prediction 3.** *If second-movers have inequality-averse preferences, they will return less in the Unequal and the Shock-Unequal treatments than in the Equal and Shock-Equal treatments. If first-movers anticipate that second-movers have inequality-averse preferences, they will send less in the Unequal and the Shock-Unequal treatments than in the Equal and Shock-Equal treatments.*

The main question addressed in our paper is whether shocks influence the levels of trust and trustworthiness. If first- and second-movers do not care about their relative endowments, whether differences are exogenously provided or by the negative random shock, they will behave in the same manner in the Unequal and the Shock-Unequal treatment. On the other hand, if the procedure that leads to different endowments matters, second-movers may feel more entitled to keep after experiencing a shock. We argue that second-movers can use their initial endowment as a reference point that may anchor their expectations, and first-movers can anticipate this behavior. We modify equation (2) to incorporate the idea of reference-dependent utility (Kőszegi & Rabin 2006, 2007, 2009; Abeler et al. 2011).

We consider that second-movers use their initial endowment as a reference point ( $r$ ) and evaluate any “gain-loss” utility from this initial endowment using the function  $\tilde{e}_2 = e_2 + \eta(e_2 - r)$ , where  $\eta \geq 0$ . Note that the endowment of the second-mover cannot be altered in the treatments without the shocks (Equal and Unequal), therefore  $e_2 = r$  in these treatments. In treatments with shocks, the initial endowment ( $r = 21$  E\$) serves as a reference point to second-movers. Thus, second-movers suffer a disutility  $\eta(e_2 - r) < 0$  when the shock is realized in the Shock-Unequal treatment. If the shock is not realized (Shock-Equal treatment), second-movers do not suffer any disutility,  $\eta(e_2 - r) = 0$ .<sup>11</sup> It follows that first-movers who expect to receive nothing from second-movers but anticipate the disutility of second-movers because of the shock will send:

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<sup>11</sup> Our functional form is a simplification of Kőszegi & Rabin (2007) or Abeler et al. (2011), where the consumption bundle ( $c$ ) is compared to a reference point ( $r$ ) and the gain-loss utility is defined by  $\mu(c - r)$ , where  $\mu(s) = \eta s$  for  $s \geq 0$  and  $\mu(s) = \eta \lambda s$  for  $s < 0$  with  $\eta \geq 0$  and  $\lambda > 1$  (this is to indicate that losses loom larger than equal-sized gains). In our setting, we focus on the loss domain because second-movers can only suffer a negative shock in the Shock-Unequal treatment, i.e.,  $e_2 - r < 0$ .

$$(7) X^* = \frac{e_1 - e_2}{4} - \frac{\eta(e_2 - r)}{4} - \frac{1}{32 \alpha_1}$$

Similarly, second-movers who are inequality-averse and have reference-dependent utility will choose the following return:

$$(8) y^* = \frac{2}{3} - \frac{1}{12 X \alpha} - \frac{e_1 - e_2}{6X} + \frac{\eta(e_2 - r)}{6X}$$

where  $\eta(e_2 - r) \leq 0$ . As a result, we expect that inequality-averse first-movers that anticipate the loss in utility of second-movers will send more to second-movers who suffered the shock than to second-movers who were initially endowed with a lower endowment; i.e., equation (7) indicates that first-movers should send more in the Shock-Unequal than in the Unequal treatment. Along these lines, we expect a smaller return in the Shock-Unequal treatment than in the Unequal treatment if second-movers have inequality-averse preferences and reference-dependent utility. If first-movers are self-interested and anticipate that they will receive less in return when second-movers suffer the shock, first-movers will send less in the Shock-Unequal treatment, compared with the Unequal treatment.

**Prediction 4.** *If first-movers are inequality-averse and believe that second-movers will suffer disutility because of the shock, they will send more in the Shock-Unequal than the Unequal treatment.*

**Prediction 5.** *If second-movers use their initial endowment as a reference point and have inequality-averse preferences, they will return less in the Shock-Unequal than in the Unequal treatment. If first-movers anticipate that second-movers use their initial endowment as a reference point and have inequality-averse preferences, they will send less in the Shock-Unequal than the Unequal treatment.*

### 3. Results

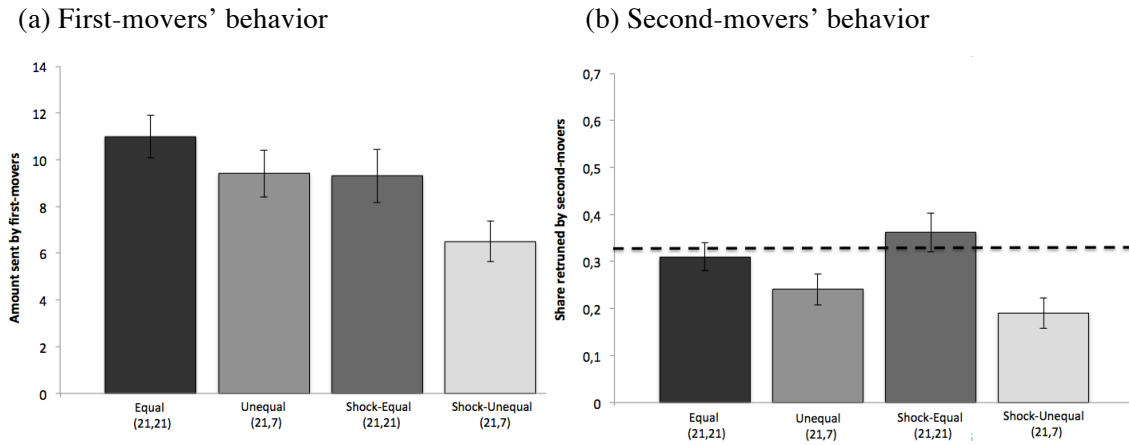
We use the average amount sent by the first-mover and the share returned by the second-mover as measures of trust and trustworthiness. Figure 1 displays these variables for each of the treatment (see also Table 1 for some descriptive statistics).<sup>12</sup> The horizontal dotted line in

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<sup>12</sup> Our analysis for second-movers focuses on those who received a positive amount from first-movers as second movers receiving nothing had no choice regarding the amount they returned. These observations are removed from the data set, leaving a total of 47 (Equal), 45 (Unequal), 39 (Shock-Equal) and 38 (Shock-Unequal) observations. In the Shock-Equal treatment, one of the second-movers decided to return all what was generated and part of her initial endowment. For this subject, we consider that her return is 1 but all our results are robust if we do not constrain this observation. The interested reader on the distributions of the amount sent and the share returned can consult Appendix B (Figures B.1 & B.2).

Figure 1(b) represents the expected return if second-movers are trustworthy and give back to first-movers what they have invested; i.e., one third of the available funds. This idea of trustworthiness follows the argument that second-movers should give back to first-movers at least what they have invested (Coleman 1990, Chaudhuri & Gangadharan 2007, Ciriolo 2007, Rodriguez-Lara 2015).

**Figure 1.** Amount sent by first-mover (left-panel) and share of available funds returned by second-mover (right-panel) in each treatment.



Notes: Error bars reflect standard errors of the mean.

**Table 1.** Descriptive statistics

	Equal	Unequal	Shock-Equal	Shock-Unequal
(a) First-movers' behavior				
Amount sent (Std. dev.)	11.00 (6.29)	9.40 (6.83)	9.31 (6.83)	6.50 (5.45)
Proportion sent (Std. dev.)	0.52 (0.30)	0.45 (0.32)	0.44 (0.34)	0.31 (0.26)
Proportion sending nothing	0	0.05	0	0.05
(b) Second-movers' behavior				
Share returned (Std. dev.)	0.31 (0.21)	0.22 (0.18)	0.36 (0.26)	0.19 (0.20)
Proportion returning nothing	0.13	0.22	0.10	0.29
N (No of pairs)	47	47	39	40

We observe that trust behavior is in line with the previous evidence in that first-movers send, on average, approximately half of their endowment (see Eckel & Wilson (2011), Johnson & Mislin (2011), Cooper & Kagel (2013)). Using non-parametric analysis, we find that the difference between the amount sent in the Equal and the Unequal treatment is not significant

( $p = 0.14$ ) but the difference between the Shock-Equal and Shock-Unequal is ( $p = 0.070$ ).<sup>13</sup> Additionally, we find no significant differences between the amount that first-movers send in the Equal and the Shock-Equal treatment ( $p = 0.12$ ), but the difference between the Unequal and the Shock-Unequal treatment is significant ( $p = 0.033$ ).

**Result 1.** *First-movers' behavior is affected by the shock. The behavior of first-movers is not statistically different in the Equal and the Unequal treatments, but they send less in the Shock-Unequal than in the Shock-Equal and the Unequal treatments.*

Figure 1(b) indicates that second-movers return a smaller share of the available funds in the Unequal treatment, compared with the Equal treatment ( $p = 0.031$ ). This finding is in line with the results of Xiao & Bicchieri (2010). We observe that such behavior occurs also in the presence of the shock, with second-movers returning less in the Shock-Unequal than in the Shock-Equal treatment ( $p = 0.002$ ). As for the comparison between the Unequal and the Shock-Unequal treatment, we find that the shock does not have a significant effect on the share returned in the unequal distributions ( $p = 0.41$ ).

**Result 2.** *Second-movers' behavior is not affected by the random shock but inequalities influence their choices. The behavior of second-movers is not statistically different in the Unequal and the Shock-Unequal treatment, but they return less in Unequal than in the Equal treatment, and do also return less in the Shock-Unequal than in the Shock-Equal treatment.*

Overall, the observed behavior from first-movers contradicts the predictions of altruistic and utilitarian preferences. We find that first-movers send less when there is an inequality that results from a random shock. We speculate first-movers' behavior might take place under the belief that second-movers are inequality-averse and have reference-dependent utility. The observed behavior of second-movers seems to suggest that they dislike inequalities as they return less when there is inequality in favor of the first-movers. However, we do not find support for the idea that second-movers used their initial endowment as a reference point after the occurrence of the shock. We argue that second-movers mainly care about the difference in the levels of endowment, no matter whether this was initially given or occurred after a shock.

Next, we show that our results are also robust to an econometric analysis. In Table 2, we report the results of two different specifications; a Tobit regression and Ordinary Least Squares. We present the analysis for the amount sent by first-movers in equations (1) and (2).

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<sup>13</sup> Our statistical analysis refers to the two-sample Wilcoxon rank-sum (Mann-Whitney) test (two-tailed) but our findings are robust to the to the robust rank-order test (Fligner & Pollicello 1981, Feltovich 2003) (see Table B.1 in the Appendix).

The analysis for the share of the available funds returned by the second-movers is presented in equations (3) and (4). The robust standard errors are reported in parenthesis.<sup>14</sup>

**Table 2.** Econometric analysis for first and second-movers' behavior

	Amount sent		Shared returned	
	(1) Tobit model	(2) OLS	(3) Tobit model	(4) OLS
Constant ( $\beta_0$ )	11.81*** (1.189)	11.00*** (0.918)	0.213*** (0.049)	0.254*** (0.042)
Unequal ( $\beta_U$ )	-1.894 (1.693)	-1.596 (1.355)	-0.103** (0.048)	-0.088** (0.040)
Shock-Equal ( $\beta_{SE}$ )	-1.689 (1.809)	-1.692 (1.458)	0.071 (0.058)	0.060 (0.051)
Shock-Unequal ( $\beta_{SU}$ )	-5.376*** (1.534)	-4.500*** (1.259)	-0.121** (0.056)	-0.099** (0.046)
Amount received			0.007** (0.003)	0.005** (0.002)
Log-likelihood	-522.567		-36.290	
Prob > chi2	0.005	0.005	0.001	0.001
R-squared		0.060		0.117
Hypothesis testing				
$H_0: \beta_{SE} = \beta_{SU}$ (p-value)	4.52 (0.035)	4.85 (0.050)	8.81 (0.003)	8.90 (0.003)
$H_0: \beta_U = \beta_{SU}$ (p-value)	4.76 (0.035)	3.89 (0.029)	0.11 (0.744)	0.08 (0.778)
Observations	173	173	169	169

Notes. Robust standard errors are reported in parentheses. Significance: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Compared with the behavior in the Equal treatment (omitted category), we observe that first-movers send significantly less in Shock-Unequal ( $p < 0.05$ ), whereas second-movers return a smaller share of the available funds in Unequal and Shock-Unequal ( $p < 0.034$ ).

Our data suggest that there are different levels of trust and trustworthiness when we compare the behavior in the Shock-Equal and the Shock-Unequal treatments ( $H_0: \beta_{SE} = \beta_{SU}, p < 0.05$ ). If we test for the effect of Unequal and Shock-Unequal being the same ( $H_0: \beta_U = \beta_{SU}$ ), we can reject this null hypothesis only for first-movers ( $p < 0.035$ ), but not for second movers ( $p > 0.744$ ). These findings confirm our Results 1 and 2 and support our previous conclusions. The level of trust is mainly affected by the occurrence of the shock (i.e., by the act of the inequality coming into existence), while the level of trustworthiness depends more on the presence of the inequality. As it is shown in columns (3) and (4), our data suggest also that the amount received by the second-mover has a predictive power in the shared returned. The estimate for the amount received is positive and significant both using the Tobit and the OLS specifications

<sup>14</sup> Our results are robust to clustering the errors at the session level.

( $p < 0.035$ ). This positive effect is predicted by our theoretical model (see equations (6) and (8) in Section 2).

#### 4. Conclusion

In this paper, we investigate whether (and how) random shocks affecting negatively the endowment of second-movers influence the levels of trust and trustworthiness. To disentangle the effects of shocks and inequality, we compare choices in a setting where the inequality is initially given with those where it occurred as the result of a random shock. This, in turn, allows us to investigate whether wealth inequalities or the nature of these inequalities is what matter most at influencing behavior in the trust game. We posit different behavioral predictions using outcome-based models and discuss the extent to which these predictions reconcile with the observed data.

Overall, we find that random shocks have a major effect in the level of trust in that first-movers send less to second-movers when the inequality results from a random shock. As for the level of trustworthiness, we find that second-movers return less when they are endowed less than first-movers, regardless of whether the difference in endowments was initially given or occurred after a random shock. Our interpretation is that second-movers are inequality-averse but do not care about the source of the inequality, thus they return less when their endowment is lower. As for first-movers, they might believe that second-movers are not only inequality-averse but do have reference-dependent utility. If first-movers believe that second-movers will use their initial endowment as a reference point, second-movers will return less when the inequality occurs after a random shock, which is consistent with our data.

Although our findings indicate that negative random shocks and inequalities might be important for the levels of trust and trustworthiness, we believe that our study is merely a first attempt in the direction of incorporating random events to study trust and trustworthiness in laboratory settings. In this respect, it would be worth analyzing the effects of negative chance events when *only* the first-mover or *both* the first- and the second-movers could be affected by the shock. In addition, the occurrence of external chance events does not always refer to negative consequences. Frank (2016) argues that positive chance events (or luck) also matters for economic success, thus a good avenue for future research would be to empirically assess how positive shocks shape behavior in the trust game. We hope our research sparks further investigation in these areas.

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## **Do negative random shocks affect trust and trustworthiness?**

**Hernán Bejarano • Joris Gillet • Ismael Rodriguez-Lara**

### **Appendix**

Appendix A: Experimental Instructions

Appendix B: Additional Results

## Appendix A

### Experimental instructions: Equal treatment<sup>15</sup>

ID: \_\_\_\_\_

A

#### Welcome to the experiment!

You are about to participate in a decision making experiment. You will be able to earn money in this experiment. How much you earn depends on your decisions and on the decisions of other participants in the experiment.

In this experiment there are two types of players. We call them A and B. Each player A will be randomly matched with a player B in the other room.

Everybody in this room has been randomly assigned to be **player A**

Except for the type of players, instructions are the same for both player A and B. Every player A and B will receive an envelope with 21 Experimental Dollars E\$. Players A get to decide first. Each A will have to decide how much of their initial E\$ – some, all, none of it – to send to the paired B. Each E\$ sent to player B will be tripled. For example (and the numbers used in these examples are picked for clarification purposes only), if player A sends 2 E\$ to the player B he/she is matched with, B will receive 6 E\$. If a player A sends 9 E\$ to his/her paired player B, B will receive 27 E\$.

Players B will subsequently have to decide how many E\$ to send back to their paired player A, keeping the remainder amount. For example (and again the numbers used in these examples are picked for clarification purposes only), if A sends 2 E\$ to B, B will receive 6 E\$. If B decides to return 5 E\$, A will end up with 24 E\$ ( $21 - 2 + 5$ ), B with 22 ( $21 + 6 - 5$ ). If Player A sends 9 E\$ to B, B will receive 27 E\$. If B decides to return 15 E\$, A will end up with 27 E\$ ( $21 - 9 + 15$ ) and B with 33 ( $21 + 27 - 15$ ).

Summarizing, the number of E\$ will be computed as follows:

E\$ Player A = 21 E\$ – E\$ sent to B + E\$ received from B

E\$ Player B = 21 E\$ + 3x E\$ received from A – E\$ sent to A

*E\$ will be converted to actual dollars at the end of the experiment (1 E\$ = \$0.5).*

---

<sup>15</sup> These are the original instructions for the first-mover (Player A) in the Equal treatment. The second-mover (Player B) receives the exact same instructions, except for the role. In the Unequal treatment, we only change the amount that corresponds to the endowment of the second-mover.

Please do not talk with the other participants during the experiment. If you need any help, or have difficulties understanding the instructions, please raise your hand and ask the instructor privately. It is important that you understand the instructions before we start.

### Experimental Procedure and Records

1. You will find an envelope with your experimental ID and 21 E\$. Please write down the same number on this sheet.
2. For each person in this room, we will draw a number from an urn with all the experimental IDs of players B in the other room. This number is the experimental ID of your paired player B. Neither you nor we will ever know more than his/her experimental ID.
3. Choose how many E\$ bills to send to your paired player B and keep the rest with you. Leave the E\$ you want to send in the envelope.
4. In the first column of **Decision Records Table**, write the number of bills you want to send.
5. We will collect all the envelopes in this room. E\$ contained in the envelopes will be multiplied by 3 and added back.
6. Your envelope will be given to the player B with the ID number randomly assigned to you.
7. Player B will count the E\$ that he/she receives and will decide how many E\$ to send back to you.
8. The envelopes will be collected and returned back to you with the amount B chooses to return.
9. Count the bills that you received inside the envelope.
10. In the second column of **Decision Records Table**, write the number of bills you received from Player B. Put all your E\$ in the envelope with your ID.
11. We will collect all envelopes in the room, and call you to exchange E\$ for U\$\$ dollars. You should present this record sheet to be paid.

**Decision Records Table**

<b>A</b>	My ID ____	My B's ID ____
I sent		I received back

## Experimental instructions: Shock treatments

### Welcome to the experiment!

You are about to participate in a decision making experiment. You will be able to earn money in this experiment. How much you earn depends on your decisions and on the decisions of other participants in the experiment.

In this experiment there are two types of players. We call them A and B. Each player A will be randomly matched with a player B in the other room.

Everybody in this room has been randomly assigned to be **player A**

Except for the type of players, instructions are the same for both player A and B. Every player A will receive an envelope with 21 Experimental Dollars E\$. Every player B will receive *initially* an envelope with 21 E\$. Players A get to decide first. Each A will have to decide how much of their initial E\$ – some, all, none of it – to send to the paired B. Each E\$ sent to player B will be tripled. For example (and the numbers used in these examples are picked for clarification purposes only), if player A sends 2 E\$ to the player B he/she is matched with, B will receive 6 E\$. If a player A sends 9E\$ to his/her paired player B, B will receive 27 E\$.

*Before* player A makes a decision about the number of E\$ to send we will roll a die in front of each player A. If the number is odd (1, 3, or 5), the amount of E\$ in the envelope of the player B to whom player A will be paired will be reduced by 14 E\$. As a result the player B to whom player A will be matched with will have an envelope with only 7 E\$ instead of the original 21 E\$. If the number is even (2, 4, or 6), the player B to whom player A will be paired keeps 21 E\$ in his/her envelope.

Players B will subsequently have to decide how many E\$ to send back to their paired player A, keeping the remainder amount. Players B will learn the outcome of the die and the amount of E\$ sent by player A before making his/her decision. For example (and again the numbers used in these examples are picked for clarification purposes only), if the reduction took places (i.e., the number was odd) and A sends 2 E\$ to B, B will receive 6 E\$. If B decides to return 5 E\$, A will end up with 24 E\$ ( $21 - 2 + 5$ ), and B with 8 E\$ ( $21 - 14 + 6 - 5$ ). If the reduction did not take place (i.e., the number was even), and A sends 2 E\$ to B, B will receive 6 E\$. If B decides to return 5 E\$, A will end up with 24 E\$ ( $21 - 2 + 5$ ), and B with 22 E\$ ( $21 + 6 - 5$ ).

Similarly, if player A sends 9 E\$ to B, B will receive 27 E\$. If B decides to return 15 E\$, A will end up with 27 E\$ ( $21 - 9 + 15$ ) and B with 19E\$ ( $21 + 27 - 15$ ) or 19E\$ ( $7 + 27 - 15$ ) depending on whether E\$ are reduced or not from their initial endowment

Summarizing, the number of E\$ will be computed as follows:

E\$ Player A = 21 E\$ – E\$ sent to B + E\$ received from B

E\$ B = 21E\$ – 14 (Reduction, if applicable) + 3x Points received from A – Points sent to A

*Remember that when players A make their decision about how many E\$ to send, once he/she knows whether the reduction in B's initial amount took place or not.*

*E\$ will be converted to actual dollars at the end of the experiment (1 E\$ = \$0.5).*

Please do not talk with the other participants during the experiment. If you need any help, or have difficulties understanding the instructions, please raise your hand and ask the instructor privately. It is important that you understand the instructions before we start.

## Experimental Procedure and Records

1. You will find an envelope with your experimental ID and 21 E\$. Please write down the same number on this sheet.
2. For each person in this room, we will draw a number from an urn with all the experimental IDs of players B in the other room. This number is the experimental ID of your paired player B. Neither you nor we will ever know more than his/her experimental ID.
3. We will roll a die in front of you and the result will determine if E\$ are reduced (odd numbers: 1, 3, or 5) or not (even numbers: 2, 4, or 6) from the initial 21 E\$ of the player B you are matched with. In the first column of **Decision Records Table**, write the result of the die and whether initial \$E are reduced for the player B you are matched with.
4. Choose how many E\$ bills to send to your paired player B and keep the rest with you. Leave the E\$ you want to send in the envelope.
5. In the envelope, you will find an **outcome card** like this:

Message from player A to player B:

- 1) I have been matched with player B's ID \_\_\_\_\_
- 2) You had a 50% probability of getting your original amount reduced in 14 \$E.
- 3) The number on the die was \_\_\_\_\_
- 4) Your original amount of 21 \$E was (reduced / not reduced) from 21E\$ to 7\$E.

Please write the outcome of the die and underline the appropriated sentence (reduced/not reduced). Put the outcome card back to the envelope.

6. In the first column of **Decision Records Table**, write the number of bills you want to send.
7. We will collect all the envelopes in this room. E\$ contained in the envelopes will be multiplied by 3 and added back.

8. Your envelope and the message will be given to the player B with the ID number randomly assigned to you.
9. Player B will count the E\$ that he/she receives and will decide how many E\$ to send back to you.
10. The envelopes will be collected and returned back to you with the amount B chooses to return.
11. Count the bills that you received inside the envelope.
12. In the second column of **Decision Records Table**, write the number of bills you received from Player B. Put all your E\$ in the envelope with your ID.
13. We will collect all envelopes in the room, and call you to exchange E\$ for U\$\$ dollars. You should present this record sheet to be paid.

**Decision Records Table**

<b>A</b>	My ID ____	My B's ID ____
The number in the die was _____		
B's initial \$E are reduced? Y / N		
I sent		I received back



## Appendix B

Figures B.1 and B.2 display the distributions of the amount sent and the proportion returned in each of the treatments.<sup>16</sup>

Figure B.1. Amount sent by first-movers in each treatment

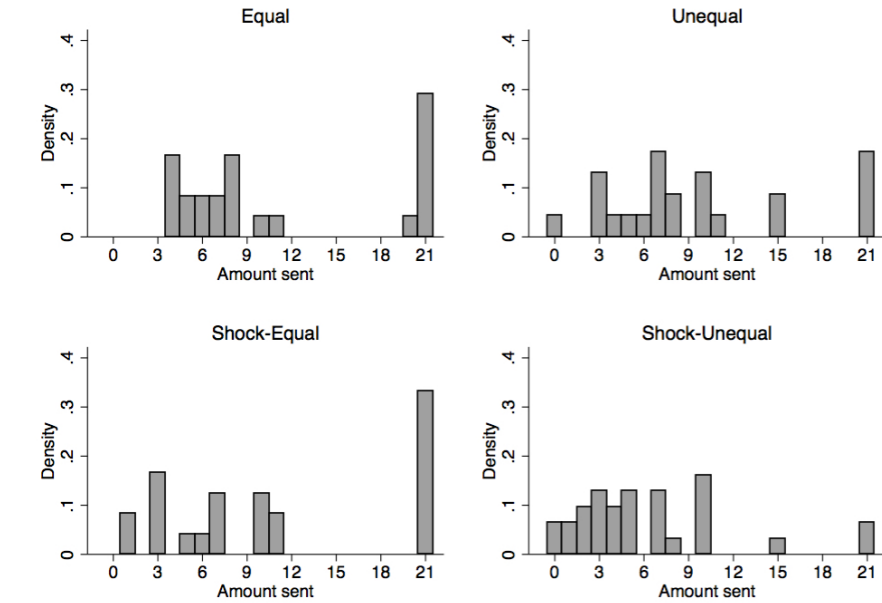
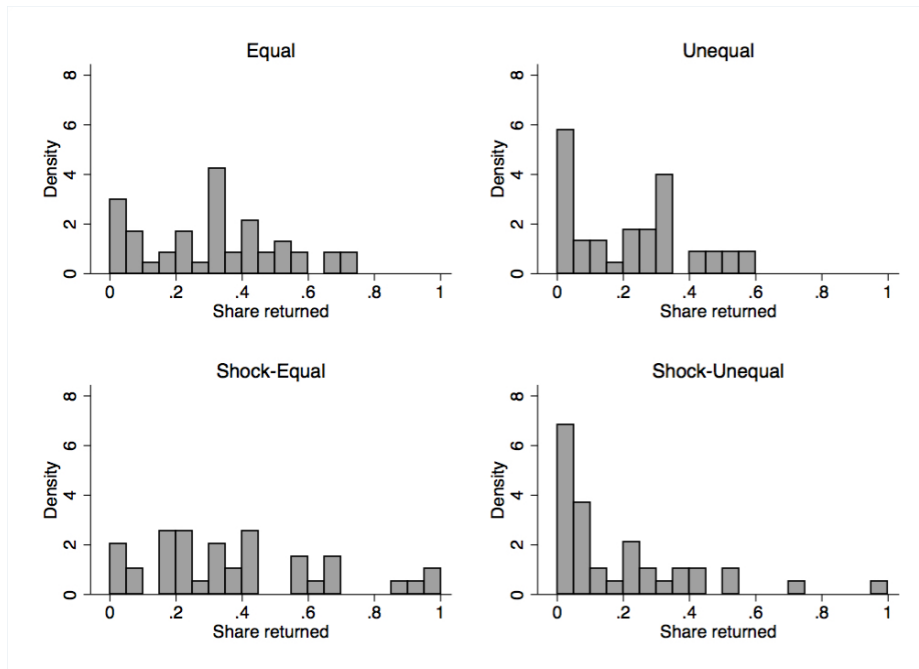


Figure B.2. Share returned by second-movers in each treatment



<sup>16</sup> In the case of second-movers, we set the number of bins to 20 (i.e., width equals to 0.05). We note that there is a second-mover who returns more than what she received in the Shock-Equal treatment. We code this observation as share return equals to 1 but all our results are robust if we do not constraint this observation.

Table B.1. Robust rank-order tests

	Amount sent	Share returned
Equal vs Unequal	1.465 (0.143)	2.220 (0.026)
Shock-Equal vs Shock-Unequal	1.847 (0.065)	3.288 (0.001)
Equal vs Shock-Equal	1.509 (0.131)	0.508 (0.611)
Unequal vs Shock-Unequal	2.199 (0.028)	0.806 (0.420)
<i>Notes.</i> <i>p</i> -values (in brackets) for two-tailed analysis.		